Thank you to the Reviewers for providing constructive comments, which have helped us to improve the manuscript. Our point-by-point responses are provided below in blue text following each of the Reviewer comments, shown in black.

Reviewer #1

This article provides some interesting datasets about the physical nature of fogs and their radiative forcing that are worthwhile of publication. Aside from technical and scientific concerns outlined below, the primary criticism is that the writing quality is of poor quality, and this obscures the potential impact of the results. It suffers from “rambling”, lacking concision that leads the reader clearly from the introduction to the conclusions. It contains many sentences that are nearly impossible to follow, statements that lack justification, and the sin of using non-quantitative descriptors like “very” and “quite”. It is not clear that anyone other than the lead author read the manuscript in detail.

We appreciate that the reviewer recognizes the value of this study. We have addressed the technical concerns as well as the specific stylistic and editorial comments itemized below. We have also edited for grammar, reduced unnecessary words and asides within the text, and removed qualitative descriptors such as those identified, as well as others (e.g., “fairly” and “relatively”). We edited the text throughout, but focused on Section 3 in accordance with the recommendations in Comment 3.

Specific items

1. p.2 l. 19. “liquid more efficiently absorbs and emits longwave radiation than ice likely increasing the forcing from liquid fog per unit mass compared to ice fogs or clear-sky ice precipitation”. Avoid words like “likely”. Otherwise, what does this mean without stating the wavelength band? Is this really true? Does it even matter that much given the size of particles is the dominant consideration determining the specific absorption?

Thank you for highlighting this sentence as problematic. Phase does influence absorptivity, but it is dependent on wavelength, as you say (see e.g., Fig. 1 in Turner 2003 [doi:10.1175/1520-0450(2003)042<0701:CPDUGA>2.0.CO;2]). The word “likely” qualifies the second clause of the sentence as a hypothesis. However, we do not test this hypothesis directly in this study. The intended purpose of the statement was to alert the reader to the fact that the microphysical and radiative properties of clouds are linked. We have reworked the sentence to be more generalized and to remove the speculative clause. We have also reordered the structure of the paragraph to improve the flow.

2. p.4 l. 26 No mention is made of size ambiguities by forward scattering probes to the assumed shape of particles. The relationship between integrated intensity in angular regime considered as a function of size is a strong function of ice crystal shape model and can result in uncertainties in measured size much greater than 100%.

The main focus of this study is on fogs that are composed of spherical liquid droplets. The sizes calculated from the FM100 measurement should be interpreted as optically-equivalent spheres. For non-spherical ice, the sizes calculated from the measurements do indeed have large uncertainties. Therefore, data collected when fogs were composed of ice is shown only in Figure 9 and only for contrast to the liquid cases. In the section of the paper where Figure 9 is discussed, we actually did note the errors associated with the sizing of ice. We felt that these errors were sufficiently important for the reader to understand that we noted them when Figure 9 was introduced instead of earlier within the methods section. Specifically, at the beginning of Section 5.1, we stated the following: *Ice particles are non-spherical and, thus, the distributions represent an effective size with reference to the scattering properties of spherical liquid; asphericity and orientation are important factors in the sizing of ice using a scattering spectrometer that impose significant uncertainties (Borrmann et al., 2000). Thus, the distributions of ice classes should be treated cautiously and are shown here for context.*

Section 3 (methods) already indicates how the calculation should be interpreted, but we agree that an explicit acknowledgement of non-spherical size uncertainties is warranted there too. Therefore, we have kept the original statements in Section 5.1, but have also expanded the discussion in Section 3 and included the Borrmann et al. reference there as well.

3. I generally cannot follow the writing or justifications for the methods outlined in Chapter 3. This section in particular needs careful editing.

We have edited this section. We have also restructured the organization to be consistent with the classification procedure and positioned these steps under subheadings.

4. p. 6 l. 25 How is it known that the clouds are single layer?

This is a reasonable assumption because we focus on fogs occurring during otherwise clear skies, but it is an assumption nonetheless. We have clarified the statement accordingly.

5. p. 7 l. 16 I do not see any reference in Shupe et al. (2013) to justify the systematic statement that reflectivities >-5 dBz are indicative of snow.

This is stated in the caption for Figure 14 in Shupe et al. (2013). This is a conservative estimate relative to Shupe et al. (2007; doi 10.1029/2007GL031008), a threshold that Shupe et al. (2013) refers to as “light precipitation” (note that it does not rain at Summit). We have clarified this in the text.

6. What justification is there to support that blowing snow does not extend above 300 m?

We do not have detailed estimates of the depth of blowing snow plumes in Greenland. However, some work has been done in Antarctica, which is the closest analogue available. Distributions of blowing snow from two Antarctic stations presented in Figure 11 of Gossart et al. (2017, doi: 10.5149/tc-11-2755-2017) show nearly all events to be less than 300 m depth. Gossart et al. reported that some events were much deeper (heights above 1000 m), but these were the exception rather than the norm and were generally found to coincide with precipitation. We have added the Gossart et al. reference to 3.2.3.

7. What is the “lofting parameterization”?

The reference is Li and Pomeroy (1997), as described in the sentence prior to the phrase in question. To improve clarity, we have replaced “lofting parameterization” with “Li and Pomeroy parameterization” and clarified the terms “lofting” and “blowing”.

8. When observations are mentioned, mention the instrument used. Presumably it is the FM100 that was used to observe particles in the lowest 10 m? Say so.

We believe this comment references the distinction between the 2 m FM100 and the 10 m FM100 rather than the distinction between the FM100s and other instruments, but it is unclear from the comment precisely where in the text this ambiguity was identified. Generally, the distinctions are clearly stated in the text where necessary, as well as in all relevant figures/captions. In some parts of the manuscript, such as the section that describes the processing of the FM100 data, no such distinction is made, but we feel that it is sufficiently clear that we are referring to both probes. We have clarified one statement where we felt there was ambiguity in the list that appears at the end of Section 3.

9. "Recall from Figure 3 that the threshold for identification used to construct Figure 5 is small (10-3 cm-3). Figure 3 also shows that the threshold at which events begin to be missed, and the rate at which missed events increase, is different for each class. For example, as expected, low density types such as snow also require a low threshold to be captured while high density types such as the fogs are relatively insensitive to the threshold." Completely incomprehensible.

This sentence was intended to demonstrate support for the classification methodology by explaining that the behavior of the lines in Figure 5 is consistent with expectations for low density (e.g., snow) and high density (e.g., fog) events. This is not a critical statement and it was removed when we edited Section 3 in response to Comment 3 and your Summary comments.

10. p. 7 l. 4 "Snow occurred more often without significant blowing snow" ??? Writing

We have rewritten the sentence in question.

11. Case studies. An allowance needs to be made for observed changes in atmospheric and fog state being due to advection into the observation region rather than the apparent default assumption that the changes are evolutions within the region due to local physical processes.

We have added this qualification to the introductory statements at the top of Section 4. Reviewer #2 asked a similar question about the effects of local topography, which we have also acknowledged in the same place.

12. p. 10 "Thus, while the fog was likely induced by radiation initially, it was maintained, and ultimately continued to grow without additional infrared loss at the surface driving saturation in the air column" The sentences that follow are generally incomprehensible but I do not understand how it could be that cloud top continued radiative cooling would not be able to maintain and deepen the fog layer. If the boundary layer cools, then the saturation mixing ratio decreases and the condensate mixing ratio increases.

The sentence in question refers to cooling from the surface, not the cloud top. We know that the surface stopped cooling because we observed the near-surface air to warm due to increased radiative forcing from the fog. Cloud top cooling is suggested later as a plausible mechanism to introduce moisture to the surface in a manner consistent with your statements. We have edited the sentences following the one in question. We acknowledge that one of these sentences (beginning "While…") was fragmented and we have fixed this. We have also added clarity and we hope that this addresses your concerns.

13. p. 12 l. 25. Referencing Hansen and Travis (1974), while correct, is a bit unnecessary given how far removed that paper is from this one. Just mention the effective diameter as being the ratio of moments of the size distribution.

We have made this change.

14. The fog bows are interesting. The physics should be described, noting in particular that the presence of fog bows does not exclude the presence of ice, only requiring the presence of liquid.

We have added a brief explanation and a reference for the optical physics of fogbows. You are correct that the occurrence of the fogbow does not rule out ice. We appreciate this suggestion and have added the qualification. We have also added an additional qualification (with references) indicating that quasi-spherical ice has been observed by others in other locations. This fact implies that we should also not rule out the possibility that under certain conditions ice fogs might produce optical phenomena similar to what is regularly observed in liquid fogs. However, this possibility is speculative for the following reasons: First, the science on habits for ice fogs is not settled (e.g., Gultepe et al. 2015, doi: 10.1016/j.atmosres.2014.04.014); second, regions such as Fairbanks, Alaska, where much of the literature on ice fogs originates is a poor analogue for Greenland due to differences in ice nucleating aerosols; and third, we are not aware of any literature specifically linking optical phenomena normally associated with liquid droplets to ice fogs.

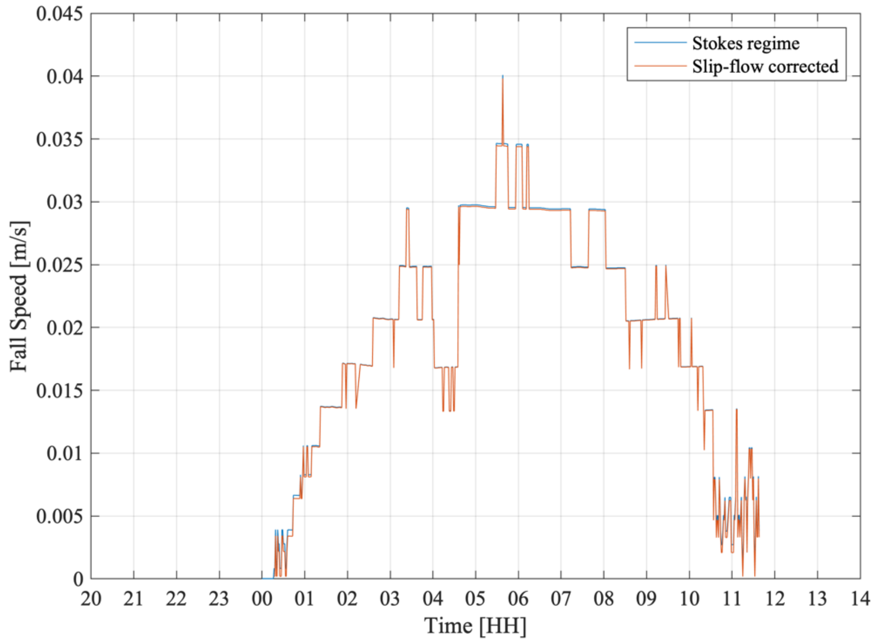
Reviewer #2

The paper provides a detailed analysis of fog hydrometeors and their effects at the Summit site from 2012 to 2014. Interesting results include the identification of liquid versus ice fogs, their microphysical properties including their time evolution and seasonality, how ice particles and super-cooled droplets can co-exist in a vertical column in the boundary layer, and their effects on radiative forcing. The paper is well-written and organized, the measurements are of scientific significance and the novel results improve our understanding of the role of cold fogs in an Arctic setting. I would accept the paper as it stands but ask the authors to address the following minor questions.

We appreciate the reviewer's positive feedback. We found the questions below to be intriguing and have endeavored to address them.

1. A fairly convincing case is made for the settling of hydrometers to explain the difference of microphysical properties between 10 and 2 m in some cases. Based on the mean particle size can you calculate the terminal velocity and see if it is consistent with the time lag identified at 2 m with respect to 10 m?

Following your suggestion, we calculated a time series of theoretical particle terminal velocities for the summertime case study, which is the case we used to estimate the settling rates you reference from the manuscript. We made these calculations following Pruppacher and Klett (2010, doi: 10.1007/978-0-306-48100-0) and the results are displayed in the figure below. Two calculations are shown: The first is based on the simple assumption of the Stokes regime (blue in the figure) and the second is corrected for slip-flow effects, which become important for small particles (red in the figure). All particles are assumed to be spherical liquid droplets and the flow is assumed to be laminar. The two theoretical calculations are similar and agree with the velocities we derived from the probes. Specifically, the theoretical calculations indicate settling rates between 0 and 0.01 m/s during the first hour of the case when we estimated ~0.01 m/s using the probe data and 0.03-0.035 m/s during the mature phase of the fog when the probes suggested a settling rate of 0.03-0.04 m/s. We decided to show the new figure here to document our work to address this question, but have elected not to add it to the manuscript. Instead, we have added two sentences to the text explaining that the settling rates implied by the time series derived from the probes are consistent with theoretical calculations, referring readers to Pruppacher and Klett (2010).



2. Although the Summit site is likely quite flat, can the authors rule out the possibility of any uplift effects when the wind direction and speed might favor adiabatic cooling based on the local but subtle topography.

This is an excellent question. Reviewer #1 asked a similar question, but referring to advection from more distant regions. The area around Summit Station is indeed quite flat. A number of detailed topographic surveys have been conducted in the area by the Cold Regions Research and Engineering Laboratory (CRREL) for logistical management of the station structures. For example, see https://www.geosummit.org/sites/default/files/docs/Summit-Station_ERDC-CRREL_TR-16-16.pdf. These surveys indicate that the maximum slope is approximately 1% at the kilometer scale. It is <1% towards the south, into the predominant wind direction from which our sampling was conducted. Locally around the station, variability in surface height is ~±2-3 m and is generally more complex than the surrounding environment because of localized drifting associated with station structures. Much of our analysis of the case studies assumes relative spatial homogeneity. Unfortunately, we cannot rule out the possibility that local topographic effects (or advection from more distant areas) might explain some of the observed time-evolution of the fogs at the location of the measurements. Therefore, we have included a statement acknowledging these limitations in the introductory remarks at the beginning of the case study section.

3. Is it possible that thermal tides, as measured by the surface barometer, might have a role in the observed diurnal signals when solar radiation is not the obvious forcing mechanism?

This is a very intriguing hypothesis! We have conducted some work linking buoyancy waves above the boundary layer to fluctuations in the thermal structure of the surface layer and associated modification of the fog microphysics. This was presented at the POLAR2018 conference in June 2018 in Davos, Switzerland, and is the subject of ongoing study (AC3-2010: http://www.professionalabstracts.com/POLAR2018/iPlanner/#/grid/1529539200). Only a small portion of that work was incorporated into the current manuscript and can be found in the discussion of Figure 7. Because we have observed buoyancy waves to create areas of convergence and divergence at the surface (see Section 4.2) that also appear to modulate fog microphysics (discussed in the poster, but not in the manuscript), it is plausible that gravitational waves, such as thermal tides could be responsible and that such phenomena could trigger condensation. We revisited the POLAR2018 study and some additional cases and believe that while the occurrence of the buoyancy waves in the case study coincides with the diurnal cycle, this is incidental and not causal. We will certainly keep this idea in mind as a plausible candidate for the missing mechanism we highlight at the end of Section 4.2, but as yet we have not found supporting evidence.